

north, in the region of the prairies. Moreover, even if the natives had known the horse, they might well have been astonished at the horse-and-man combination.¹ Then it is difficult to understand why *Equus* became totally extinct, since subsequent events showed that vast areas were admirably adapted to it. Prof. Ewart informed me (*litt. 1902*) that the Chinese were alleged to have visited America about the eleventh century, and reported it as the "land of women, the horse and the vine." This tradition is apparently not to be regarded very seriously, but the antiquity of the genuine *Equus caballus* in North America is supported by O. P. Hay in his excellent catalogue of the fossil Vertebrata of North America (1902), p. 622. This author boldly lists *E. caballus* as Pleistocene on this continent, and while admitting that "in some cases the identifications have been open to question," and "in other cases the remains may have been derived from the introduced race," he adds, "the former existence of the species in Alaska and in California appears well established." Of course, the term *E. caballus* must here be understood in the wider sense. Prof. Ewart also remarked, in the letter just cited, that the Spaniards at the time of the conquest used small-headed horses, "the offspring of the *E. fossilis* of Asia in all probability," whereas the characteristic "buckskin" pony of our south-west is a relatively large-headed animal. Furthermore, Mr. Wilfred Blunt, through my brother, Mr. S. C. Cockerell, communicated the statement that "the Spaniards never rode mares, and can hardly have brought any but stallions with them in their ships to their colonies." Hence the early abundance of wild horses in North and South America appears very remarkable. With reference to the presumed early absence of horses, one may also remark that so common an animal as the "antelope" (*Antilocapra*) was not made known to naturalists until about 1815, and a perfectly new wild sheep was discovered in northern Mexico in 1901! Even the known variability in colour of the wild horses might be thought of as a Mendelian phenomenon, resulting from the mixture of different types, and the infusion of new blood could be conceived to have resulted in greater vigour and consequent increase in numbers.

T. D. A. COCKERELL.

Colorado Springs, Colorado, U.S.A.

THIS paper, only slightly abridged and with about one-third of the figures, appeared in NATURE of April 21 (vol. lxix. p. 590). I am more than "half inclined to regard the Celtic pony as a valid species" and to recognise three species of living horses. I prefer, however, to leave systematists to decide whether Prjevalsky's horse and the Celtic pony should be regarded as species or merely as varieties.

J. C. E.

Entropy.

AN author expects some unfavourable reviews, and, if wise, profits largely by them; but Prof. Perry's review of "Entropy" in NATURE of April 14 is simply an attempt to brush away a book the object of which is to eradicate what, I submit, is a very widespread mistake, because the reviewer has himself not only made the mistake, if mistake it be, but championed it. This mistake is that entropy is conservative in irreversible change; that the entropy of a body is increased only by its taking in heat, or that $\int dH/\theta$ is the entropy in irreversible change; or that dH/θ is a complete differential. In Prof. Perry's own words, "There is a property of the stuff called its entropy ϕ , which is such that any change in it, $\delta\phi$, if multiplied by t the absolute temperature gives δH or $\delta H = t\delta\phi$." " ϕ is to heat received H something like what v is to work w ." "If we divide every δH by t , . . . every amount δH being divided by the t at the time, and if we call δH divided by the t by the name, entropy, we shall find that when the stuff is brought back to its old state again, we have just given out as much entropy as we have taken in. The account balances exactly."

In a note to a presidential address I pointed out that such statements are numerically correct in reversible changes

¹ Some of the aboriginal pictographs show horses, but these are apparently of recent date. Unfortunately we have no ancient American drawings of animals comparable to those of Europe.

only, that in all irreversible changes they are not accurate, and that they thus give a wholly wrong idea of the function entropy. There was no question, and never has been, about reversible changes, that is to say, changes where \dot{p} and θ are uniform throughout the working substance; the whole of my criticism refers to irreversible changes alone.

Prof. Perry then started a correspondence in which Prof. Poincaré and Prof. Planck were good enough to join, and also showed how Prof. Perry was wrong (*Electrician*, March 13, 1903). I quote from Prof. Planck's letter:—

"The controversy excites my attention the more, when, to my astonishment, I see a man so well known and so eminent in science as Sir Oliver Lodge¹ putting forward ideas on thermodynamics (*Electrician*, January 23, p. 460) which I combated ever since the commencement of my studies in that science."

"But how can I hope with my words to make any impression on such writers when Mr. Swinburne's excellent articles have failed to effect any change in their preconceived ideas? For, with one reservation,² what he has written in the *Electrical Review* (January 9, p. 52) is, in my opinion, one of the best and clearest expositions of the subject that has ever been written, especially where he points out that Nature never undertakes any change unless her interests are served by an increase of entropy, while man endeavours so to make use of those changes allowed by Nature that his own interests—namely, the acquisition of available energy—are served as completely as possible."

Science can never be a matter of authority, but I quote Prof. Planck because Prof. Perry now reviews the book as if his definition of entropy was universally accepted in thermodynamics, and adopts the tone that anyone who differs from himself and develops Clausius's inequality, $\int dH/\theta < \phi$ for all irreversible changes, is wrong *prima facie*.

Though the review contains quotations from the little book, they are always incomplete, so as to give as far as possible an absurd meaning. Thus the quotations about errors in text-books look as if I said text-books on thermodynamics are wrong. What I do say is that books on physics and steam engines define ϕ as $\int dH/\theta$, whereas

books on thermodynamics show that is accurate for reversible changes only. The whole gist of my book is the application of Clausius's principle of increase of entropy. Books on steam engines, and generally on physics, as opposed to those on thermodynamics, say $d\phi = dH/\theta$, and dH/θ is a complete differential. If θ means the temperature of the working substance when that temperature is not uniform, dH/θ has no meaning, and is not a complete differential. By θ in irreversible change, as I have often explained, I mean the temperature at the separating surface through which dH passes. If no meaning can be given to dH/θ in irreversible change, my criticism that dH/θ is not a complete differential, except in the ideal case of reversibility, is still valid. "It is hardly believable that in a dynamical illustration he should imagine the momentum of a system of two colliding bodies to be increased by the collision" is calculated to give the impression that I am ignorant of elementary mechanics. The context is discussing the sum of the scalar momenta of gas particles. This increases when some isolated gas equalises its temperature at constant volume. "But as we have the foot-pound,

¹ The reference to Sir Oliver Lodge occurs because he wrote an article on entropy defined so that $H = \int \theta d\phi$, which I take it he has recalled. It was because I thought the weight of his authority might tell harmfully that I sent the correspondence to two leading authorities on thermodynamics.

² This was my statement that $d\phi$ is never a complete differential in irreversible change. For $d\phi$ to be a complete differential in terms of, say, $d\theta$, dv , we must have $d\phi = M d\theta + N dv$, where $\partial M/\partial v = \partial N/\partial \theta$. To prove $d\phi$ a perfect differential during any irreversible change the equation must be true while the change is going on. It is not accurate to put the value of θ or of ϕ which obtained before the change started, or would be reached if the change were arrested and the substance allowed to come to uniform temperature and pressure. Prof. Planck is so much better a physicist and mathematician than I am that I do not contradict such an authority; I merely say there is a misunderstanding, which may be mine, and I submit my contention. My view is that the physical meaning of a complete differential in mechanics is not only that the integral is completely determined by the coordinates, but that it is conservative. Lagrange's treatment of mechanics really involved the conservation of energy, that is to say of the forms he discussed.

and I think the poundal, as units of energy" looks as if I confuse force and energy. The context shows that I object to non-metric units as unscientific, and therefore do not care which unit bears the name poundal. The statement that I want to have Claus instead of Rank for the British unit of entropy is wrong. The claus is the unit of entropy in the practical metric system where the joule is the unit of energy.

The rank is a name proposed by Prof. Perry for $\int dH/\theta$, and as this is not entropy in any real change, I cannot adopt it as a unit of entropy. As to $d\chi$, I will deal with that elsewhere; it is a side issue. The statement that I talk of "the entropy of a quantity of heat" is wrong. Prof. Perry holds that entropy is a factor of heat. I dissent, and agree with Prof. Planck that entropy is not a factor of energy. So far from talking of the entropy of a quantity of heat, I have explained very fully how and why entropy is in no sense a factor of heat.

I would not write were a review in NATURE not particularly important, and I trust you will, in fairness to my publishers and myself, allow this letter to appear.

41 Palace Court, W., May 1. JAMES SWINBURNE.

My sole object in the controversy to which Mr. Swinburne refers was to show that, like most of the other writers of whom he complained, I have never either made or championed the mistakes he speaks of at the beginning of this letter. As to my notice of his book, I cannot admit that I have misrepresented him except as to the *claus*. I made a mistake in saying that his *claus* is what is sometimes called a *rank*. As he now says that the momentum of which he spoke was a *scalar* momentum, I submit that I was quite fair in my comments. I cannot admit that his $d\chi$ diagram is a side issue. JOHN PERRY.

Origin of Plants Common to Europe and America.

THAT there is a resemblance between the floras of Canada and northern Europe, and again between the floras of Canada and of eastern Siberia and Japan, is well known. Including the horsetails and ferns with the flowering plants, probably about 575 species are identical in Canada and Europe, and again about 330 in Canada and Japan or the River Amur country. A large number of these are common to the three continents. The hypothesis generally accepted has been that, in some comparatively recent epochs, there has been a connection between Europe and America which facilitated the intermingling of the plant life of the two continents. The late Prof. Asa Gray suggested the probability that the migration of European plants had taken place across Asia to America. Lesquereux, from his studies of the flora of the Dakota group, on the other hand, maintained that the North American flora is not now, nor has it been in past geological ages, the result of migration, but that it is indigenous. It has long been known that species now extinct occurring in the Miocene of Europe had appeared in America at an earlier period. Lester Ward enumerates eleven species—all now extinct—as common to the Laramie group in the United States and the Eocene of Europe, and shows further that at least two living species now found in both Japan and America date their origin in America as far back as the Eocene. Twenty years ago my own studies in the distribution of Canadian plants also convinced me that whilst facilities had existed for migration in both an easterly and a westerly direction, Canada was the point of origin of many of the species now identical in Europe and America. This conviction has been heightened by further knowledge of the range in Canada of these identical species and by further discoveries during recent years of plants in the Pleistocene clays of Canada. Of seventy fossil species in these Pleistocene clays at Toronto, Ottawa and elsewhere, twenty occur at the present day in both Europe and Canada, fourteen are similarly Asiatic and Canadian, whilst eleven are common to the three continents. This, if it does not necessarily indicate that in Pleistocene times the intermingling of these floras had already been effected, at least shows that in this period these identical species were present in Canada, and had

here their place of origin if there is nothing to indicate their presence at as early a period in Europe or Asia. In its vast areas of exposed Laurentian and Huronian formations, Canada has an old look about it, and must have furnished a home through long past ages for the growth and diffusion of northern temperate plant life, when other sections of the globe have from time to time been under water.

The peculiarities of the present range over Canada of many of these identical species also afford suggestions. Whilst many of them are distributed somewhat generally over the country, and many are high northern or Arctic, quite a number do not range west of Lake Superior; others have not been found west of the Rocky Mountains, whilst some are confined to British Columbia and Alaska. In view of their occurrence also in either Asia or Europe, this circumscribed range of so many species suggests their antiquity, and that the elevation of that lofty barrier, the Rocky Mountains, and the disturbance of the relations of land and water in Manitoba and the North-West Territories in more recent times, has resulted in these plants being confined to their present range where forest conditions were more suitable, and has led to the treeless prairies and plains being tenanted by new groups of species specially suited to the new conditions there, when the land rose to its existing level.

A. T. DRUMMOND.

Toronto, April.

Moisture in the Atmosphere of Mars.

IN your issue of May 5 I see a note in the astronomical column on Mr. Lowell's theory of the Martian canals. It is perhaps not just to criticise it on so short a summary, but there is a point on which I should like to ask a question. If, as Mr. Lowell says, there is not sufficient moisture on the planet to produce vegetation, how does the water return to the poles ready for the next summer? The only way, it seems to me, is by evaporation. His suggestion of artificial waterways to carry the water from the polar caps implies the existence of an atmosphere sufficiently dense to enable intelligent beings to live. That being so, is it not just as plausible that the evaporated water should condense in the form of rain on the general body of the planet as well as at the poles? although, of course, the excessive cold would account for an increased fall at these extremities.

Bournemouth, May 10.

ARTHUR J. HAWKES.

Radium and Milk.

IN the souring of milk the amount of lactic acid developed may reach 0.80 per cent. in three or four days when the milk solidifies. In view of Sir O. Lodge's suggestion (NATURE, October 1, 1903), I have made experiments comparing the rate of acidification, in two to three days, with and without the influence of radium rays from a 5 mgrm. radium bromide tube. The differences in five cases did not exceed the limit of experimental error, 0.01 per cent. of lactic acid, and in a sixth case with the milk solidified the difference only amounted to 0.05 per cent. of lactic acid. It therefore appears to me that under normal conditions radium rays have little or no effect on the functions of the lactic acid bacillus.

WILLIAM ACKROYD.

Halifax.

THE BANTU RACES OF SOUTH AFRICA.¹

NOTHING so good as this book dealing with the Negro indigenes of southern Africa has yet appeared. Mr. Dudley Kidd's work is therefore entitled to take the first rank on this subject, at any rate as far as the Bantu races of South Africa are concerned.

It is a national humiliation to us to reflect that as a Government we have been connected with South Africa for more than a century, that is to say, two-thirds as long as our imperial connection with India has lasted, and yet that by Government endeavour or

¹ "The Essential Kafir." By Dudley Kidd. Pp. xiii+436. (London A. and C. Black, 1904.) Price 18s. net.